EMPLOYMENT EFFECTS OF MINIMUM AND SUBMINIMUM WAGES: PANEL DATA ON STATE MINIMUM WAGE LAWS

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Using panel data on state minimum wage laws and economic conditions for the years 1973–89, the authors reevaluate existing evidence on the effects of a minimum wage on employment. Their estimates indicate that a 10% increase in the minimum wage causes a decline of 1–2% in employment among teenagers and a decline of 1.5–2% in employment for young adults, similar to the ranges suggested by earlier time-series studies. The authors also find evidence that youth subminimum wage provisions enacted by state legislatures moderate the disemployment effects of minimum wages on teenagers.

A federal minimum wage was first implemented in the United States with the passage of the Fair Labor Standards Act of 1938, which now covers more than 90% of all workers. Since its enactment, there has been widespread debate about the merits of minimum wage laws, along with numerous efforts to evaluate their economic effects. By the early 1980s, the amassed body of theoretical and empirical research by economists, including that of the Minimum Wage Study Commission (1981), suggested that the imposition of minimum wages decreases employment opportunities for workers with wages at or near the minimum wage. More explicitly, a 10% increase in the minimum wage apparently reduced teenage employment by 1% to 3%, with proportionately smaller effects for 20–24-year-olds, reflecting their smaller representation in the minimum wage population (Brown et al. 1982; Brown 1988).

An important shortcoming of that body of research, however, is that the results were based almost exclusively on time-series data on the federal minimum wage, on workers covered by the federal minimum, and on the aggregate labor market. The authors of these time-series minimum wage studies often recognized the inadequacy of their data for empirical analysis of minimum wage effects (for example, Wachter and Kim 1978). Yet, out of the vast number of U.S. minimum wage studies, only about ten do not resort to time-series data.

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In this study, we augment the national time-series data with information on geographic differences in the minimum wage. Individual states have frequently legislated changes in the level or coverage of the minimum wage, and although states cannot impose a minimum wage lower than the federal minimum (for workers covered by the federal minimum), many states have periodically raised their minimum wage above the federal level or extended coverage to workers excluded from federal legislation. In addition, states with minimum wages above the federal level have sometimes implemented exemptions from the higher state level for specific subgroups of the labor force, such as teenagers or students. Using this information, we construct a panel data set on minimum wage laws and labor market conditions at the state level for the years 1977–89 (or, for some states, 1973–89) to reevaluate the existing evidence on the consequences of minimum wages for the youth labor market. In our view, this integration of state data into the analysis of minimum wage effects makes three contributions to the literature on minimum wages.

First, studies using the federal minimum wage in time-series analyses face a number of potential problems. The minimum wage variable used in these studies exhibits relatively little variation over time, and what variation exists is correlated with changes in government social welfare and training programs, and with the effects of the draft; this problem makes it difficult to isolate the effects of minimum wages. Moreover, state minimums are sometimes set above the federal level. A few states nearly always set minimum wage levels above the federal level; and in the latter part of the 1980s, as many as 25% of states had minimum wage levels above the federal level. The federal minimum wage variable used in past time-series studies therefore measures the effective minimum wage with error; this may be particularly severe in the latter part of the 1980s. In contrast, the data on state minimum wage laws and economic conditions permit a pooled time-series cross-section analysis of minimum wage effects, exploiting the greater independent variation in relative minimum wages at the state level, and avoiding the measurement error caused by using a uniform federal minimum wage.

Second, the use of panel data addresses an important criticism leveled at the small number of previous studies that used single cross-sections (in part to address some of the problems with time-series analyses of minimum wage effects). Most of these studies included either a dummy variable for the existence of a minimum wage law (Katz 1973; Freeman 1982) or data on state levels and coverage (Cotterill and Wadycki 1976; Welch and Cunningham 1978). As a result, much of the variation in the included minimum wage variable (typically the minimum wage level, multiplied by coverage, divided by an average wage) arose from variation in average wage levels across states, leading critics to argue that the estimated wage effects largely reflected state "average wage" effects rather than minimum wage differences (Brown et al. 1982). In contrast, the use of panel data permits explicit estimation of state (or year) effects as distinct from the effects of changes in the minimum wage variable, and so permits a cleaner evaluation of minimum wage effects from the cross-sectional (and time-series) variation in the data. In addition, considerable variation in the minimum wage variable comes from changes in legislated minimum wages at the state level during the latter part of the 1980s.

Third, the use of state data permits a direct evaluation of the effects of lower minimum wage levels for subgroups of the population. The latest federal minimum wage legislation—effective in April 1990—attempted to alleviate the adverse effects of the minimum wage on the employment of youths by introducing, for the first time at the federal level, a "subminimum" wage for newly employed workers. Because a broad federal youth subminimum had never before been implemented, however, aggregate time-series data for the U.S. economy cannot be used to evaluate the potential for a subminimum to mitigate the disemployment effects of the minimum wage.[1] In contrast, numerous states have implemented subminimums for young workers or students in years past, so data on state legislation can be used to examine the effects of subminimum wage provisions.

Some recent research has challenged the finding of the earlier time-series studies that increases in minimum wages reduce employment of teenagers and young adults, with elasticities in the range of −0.1 to −0.3. Wellington (1991) argues that the addition to the sample period of the 1980s (through 1986), during which the minimum wage fell sharply in real terms, has the effect of reducing the estimated disemployment effects. For a hypothesized 10% rise in the minimum wage, she reports disemployment effects of less than 1% for teenagers and essentially zero for young adults. Card, in "Do Minimum Wages Reduce Employment?" (in this issue), compares the employment experiences of Californian workers during the late 1980s, when the state minimum wage level rose sharply, with the experiences of similar workers in labor markets with no changes in minimum wage laws. He finds no evidence of a disemployment effect from the higher minimum wage. In his other paper in this issue ("Using Regional Variation in Wages . . . "), Card draws a similar conclusion from a short panel data set on all states, for the period 1989–90. Finally, Katz and Krueger, in a survey of fast-food restaurants in Texas (also in this issue), find no evidence of declines in employment in response to the 1990 increase in the federal minimum wage, at least in establishments that remained in operation following the minimum wage increase. In contrast, our estimates corroborate the range of estimates from existing time-series studies (Brown et al. 1981), with elasticities concentrated from −0.1 to −0.2. We conduct numerous sensitivity analyses to attempt to reconcile the apparently conflicting findings.

The State Minimum Wage Panel Data Set

The panel data set of minimum wages, minimum wage coverage, and local economic conditions consists of annual observations covering the 50 states and the District of Columbia for the years 1977 through 1989, and extending back to 1973 for a subset of 22 larger states for which the CPS identified state of residence from 1973 through 1976. We obtained from each state labor department a chronology of applicable minimum wage legislation dating back to the early 1970s. These data were cross-checked against information available from the Bureau of National Affairs' Compensation Primer and the chronologies constructed by Questor (1981).[2]

In most cases, this procedure yielded a single time series of the minimum wage level, from which we extracted the value in effect during May of each year (for consistency with the data on state labor market conditions described below). For a few states, however, the state minimum wage level differed by occupation or labor force group, and additional steps were necessary to obtain a single value that best captured the effective state law. Where the varying levels clearly were subminimums for youths, students, newly covered workers, or very low-skilled occupations, we used the highest value in each year. In two

[1] Past legislation sometimes permitted some classes of employers (primarily colleges and universities) to pay subminimum wages to full-time students, but had never before been generalized to all young or new workers In a study for the Minimum Wage Study Commission, Freeman et al. (1981) analyzed this specific provision using a data set constructed from records of private sector employers certified to hire full-time students. Their main focus, however, was on the micro-level variables associated with the employment of workers under this subminimum wage Brown et al (1983) found that adjusting the Kaitz index of the minimum wage for coverage exemptions for students had no discernible impact on time-series estimates of minimum wage effects

[2] Our principal focus is on legislated minimum wage levels, rather than state laws extending minimum wage coverage As explained below, estimates of coverage by state laws are hard to come by. In addition, in the few time-series studies that consider the effects of coverage separately from the effects of (relative) minimum wage levels, coverage effects tend to be weaker (Brown et al. 1982).

other cases, the existence of multiple minimum wage levels was not automatically suggestive of a subminimum. In the District of Columbia there are nine separate minimum wage levels for different industries and occupations, as well as differing minimum wage levels for youths, students, and JTPA workers. In this case, we used the weighted average of the minimum wage levels for adults across the nine categories, weighted by estimated employment in each category in each year. In Minnesota in recent years, the state minimum wage level for workers covered by the federal law (FLSA) differed from the level for those covered only by state law; in this case, we used the state minimum for workers covered by the FLSA.[3]

Table 1, column (1) shows the states with legislated minimum wage levels above the federal level for each year in our sample, along with the legislated levels. Throughout the 1970s and much of the 1980s, only a few states set a minimum wage above the federal level.[4] By 1989, however, the number of states with higher minimums had risen to 13.[5] Columns (2) and (3) report the federal minimum wage level for each year and the (unweighted) average percentage difference between the state and federal minimum wage levels for states with legislated minimum wage levels exceeding the federal level. Perhaps not surprisingly—given that states tended to raise their minimum wages when the federal minimum wage was stagnant—the average percentage differential is greatest when the number of states with minimums exceeding the federal minimum is largest, rising to a peak of 16% in 1989.

In addition to minimum wage levels, for each state we collected information on the existence of subminimum wage provisions that permit employers to pay a lower wage to specific subgroups of the labor force. Generally, subminimum wage provisions enacted by state legislatures in the past have taken two forms: a subminimum (or exemption) based on age; or one based on student or apprentice (learner) classification. As shown in Table 1, most states with minimum wages above the federal level had subminimum wage provisions in their minimum wage legislation. Columns (6) and (7) of Table 1 also show that over the sample period, about half of all states have had youth subminimums, and two-thirds of all states have had student/apprentice subminimums.

Comprehensive time-series information on state minimum wage coverage was more difficult to assemble. For the federal law, the Department of Labor has published estimates of the number of wage and salary workers in each state by their coverage status under the minimum wage provisions of the FLSA for most years in our sample. For coverage by state laws (above and beyond FLSA coverage), data are available from the Department of Labor only for the years 1974, 1975, and 1977; as a result, we use the FLSA coverage estimates for each state for the available years. For years with no official estimates (1979–81), we assumed that federal coverage on a state-by-state basis changed in proportion to the change in coverage for the United States as a whole. For the years after 1986 (the latest data available), we assumed that coverage rates held steady at their 1986 level. Because FLSA coverage differs across states, this approach seems preferable to using just data on minimum wage levels, and should help to reduce measurement error in the effective minimum wage.

[3] This difference is relevant in Minnesota because the state minimum wage levels for both groups were higher than the federal minimum wage in 1988 and 1989 New legislation effective in 1991 eliminated this two-tier schedule, although the new structure sets a higher minimum wage for large employers than for small employers (who are less likely to be covered by the FLSA).

[4] Legislation in both Connecticut (beginning in 1974) and Alaska (beginning in 1977) automatically keeps the state minimum wage above the federal level In Alaska, a constant differential of 50 cents per hour is maintained. In Connecticut, the law through 1987 set the state minimum .5% above the federal level, resulting in a differential of just a few cents.

[5] With the increase in the federal minimum to $3.80 per hour in 1990 and $4 25 in 1991, the number of states with higher minimum levels has again dropped, to five in 1991.

Table 1 State Minimum Wage Levels, 1973–89.

			Average Coverage-Adjusted Relative Minimum Wages		*Proportion (Number of Changers) with Subminimums*	
States with Minimum Wages Exceeding Federal Minimum[a] (1)	*Federal Minimum* (2)	*Average % Difference Between State and Federal Minimum* (3)	*State Minimum > Federal Minimum* (4)	*State Minimum = Federal Minimum* (5)	*Student/ Apprentice* (6)	*Youth* (7)
1973 CA^{sy} CT^{sy} DC^{sy} MA^{y} NJ^{sy} NY^{y} 1 65 1 85 2.16 1.85 1 75 1 85	1.60	15.7	.30	.31	.55	.64
1974 CT^{sy} DC^{sy} 2 01 2.19	2.00	5 0	29	36	59 (1)	.59 (1)
1975 CT^{sy} DC^{sy} NJ^{sy} 2 11 2 45 2 20	2.10	7.3	30	.35	.59 (0)	.55 (1)
1976 CT^{sy} DC^{sy} HI^{s} 2.31 2 55 2 40	2.30	5 2	31	.36	.59 (0)	.55 (0)
1977 AK^{sy} CA^{y} CT^{sy} DC^{sy} HI^{s} NJ^{sy} 2 80 2 50 2.31 2.76 2 40 2.50	2 30	10 7	.33	35	63 (2)	.51 (1)
1978 AK^{sy} CT^{sy} DC^{sy} 3 15 2 66 2.79	2 65	8 1	.33	38	.65 (1)	53 (1)
1979 AK^{sy} CT^{sy} DC^{sy} 3 40 2.91 2.95	2 90	6 4	33	38	63 (1)	55 (1)
1980 AK^{sy} CT^{sy} DC^{sy} 3.60 3 12 3.14	3.10	6 0	32	.38	63 (0)	.55 (0)
1981 AK^{sy} CT^{sy} DC^{sy} 3 85 3 37 3 48	3.35	6.5	.34	38	63 (0)	55 (0)
1982 AK^{sy} CT^{sy} DC^{sy} 3.85 3 37 3 62	3.35	7 9	.30	.35	59 (2)	53 (1)
1983 AK^{sy} CT^{sy} DC^{sy} 3.85 3.37 3.82	3.35	9.9	32	.34	.59 (0)	.53 (0)
1984 AK^{sy} CT^{sy} DC^{sy} 3 85 3.37 3 82	3 35	9 9	.32	33	.59 (0)	.53 (0)
1985 AK^{sy} CT^{sy} DC^{sy} ME^{s} 3.85 3 37 3.85 3 45	3.35	8.4	.35	.35	59 (0)	53 (0)
1986 AK^{sy} CT^{sy} DC^{y} ME^{s} 3.85 3.37 3.86 3.55	3 35	9 2	.33	34	.67 (6)	.53 (0)
1987 AK^{sy} CT^{sy} DC^{y} MA^{sy} ME^{s} NH^{sy} 3.85 3 37 4.16 3 55 3 65 3.45 RI^{sy} VT^{sy} 3 55 3 45	3 35	8 3	34	34	.67 (0)	.53 (0)
1988 AK^{sy} CT^{sy} DC^{y} HI^{s} MA^{sy} ME^{sy} 3.85 3 75 4.33 3.85 3 65 3.65 MN^{sy} NH^{sy} RI^{sy} VT^{sy} 3.55 3.55 3.65 3 55	3.35	11 6	.33	.33	.67 (0)	53 (0)
1989 AK^{sy} CA^{sy} CT^{sy} DC^{y} HI^{s} MA^{sy} 3 85 4.25 4.25 4.33 3.85 3.75 ME^{s} MN^{sy} NH^{sy} PA^{s} RI^{sy} 3.75 3 85 3 65 3.70 4.00 VT^{sy} WA^{sy} 3.65 3.85	3.35	16.5	.33	.32	.67 (0)	.55 (1)

[a] States with postal codes underlined had relative coverage-adjusted minimum wages in the upper quartile High-wage states with student/apprentice subminimum wage provisions are denoted with an "s" superscript, and those with youth subminimum wage provisions are denoted with a "y" superscript.

Sources State labor departments, U.S. Department of Labor, *Minimum Wages and Maximum Hours Standards*, various issues; and authors' calculations from Current Population Survey microdata tapes.

For each state-year observation, a coverage-adjusted minimum wage is computed as the product of the greater of the federal or state minimum wage, and federal coverage for the state. We then computed, for each state, the ratio of the coverage-adjusted minimum wage prevailing in May of each year to the state average hourly wage during the same month; this is the variable used in the analysis.[6] Columns (4) and (5) of Table 1 provide more information on the role of state minimum wage laws in influencing effective minimum wages. These columns report the average coverage-adjusted relative minimum wage variable separately for states with minimum wage levels exceeding the federal level and states in which the federal minimum wage level is binding.

A comparison of columns (4) and (5) reveals that minimum wage levels higher than the federal minimum generally did not result in higher relative minimum wages; indeed, throughout most of the sample period, relative minimum wages were higher in states *without* minimum wage levels exceeding the federal level, reflecting the lower average market wage in those states. This pattern is also revealed by the small number of states with minimum wages exceeding the federal minimum that were in the upper quartile of the distribution of the coverage-adjusted relative minimum wage in each year; these states are underlined in the first column of Table 1. Over the 1980s, however, the average relative minimum wage in states in which the federal minimum wage was binding declined, and by the end of the sample period, the average relative minimum wage was roughly the same for both sets of states. Thus, when evaluated in terms of changes, the rising incidence of state minimum wage laws did boost relative minimum wages during the 1980s.

For data on state labor market conditions over the same period, we also used the May files of the Current Population Survey (CPS). Variables estimated from the CPS include employment rates for teenagers (aged 16–19) and young adults (16–24); unemployment rates for prime-age (25–64) men; proportions of the population aged 16–19 or 16–24; and the proportions of individuals aged 16–19 or 16–24 enrolled in school. In all cases, the variables are calculated from the individual survey responses, aggregated to the state level using the CPS demographic weights.[7]

Empirical Evidence on Minimum Wage Effects

Reconsidering the Existing Evidence

As noted above, nearly all past minimum wage studies have focused on time-series data at the national level. In particular, the typical time-series study estimates a regression equation of the form

$$(1)\quad E_t = \alpha_0 + \alpha_1 MW_t + X_t\beta + \epsilon_t.$$

E is the employment-to-population ratio for the age group under study;[8] MW is a cov-

[6] Because state-specific wage rates are not published outside of manufacturing, the average state wages used are estimated as the mean usual hourly wage from the May Current Population Surveys (CPS) We chose the May CPS because, prior to 1983, the questions pertaining to usual weekly hours and earnings were only asked in May For all data computed from the CPS, we deleted persons under age 16, self-employed workers, unpaid family workers, and persons indicating agricultural production or agricultural services as their current or most recent industry.

The ratio of the coverage-adjusted minimum wage to the average wage for the age group studied may better indicate how much the minimum wage cuts into the wage distribution. For many states, however, the cell sizes from which we can compute mean wages for teenagers and young adults are quite small (especially after 1982, when wage information was elicited from only one-fourth of the sample) In addition, the average teen wage is heavily influenced by the minimum wage

[7] Madden (1991) reports that the January 1985 reweighting of the CPS, when data from the 1980 U.S. Census were incorporated, resulted in shifts toward more prosperous households (presumably reflecting the shift of the population from urban to suburban areas), with possibly different employment rates. The individual-year dummy variables that we include should capture the effects of this reweighting.

[8] We focus on the employment-to-population

erage-adjusted minimum wage variable; X is a set of variables to capture aggregate business-cycle effects, the changing age structure of the population, and, in some specifications, school enrollment rates; and the "t" subscript indicates the year or quarter that the data describe. Although employment and wages are determined by the interaction of labor supply and labor demand, this equation is assumed to represent a reduced form capturing the effects of exogenous variables on equilibrium employment.[9] Existing evidence on minimum wage effects estimated from specifications like equation (1) indicates elasticities in the range of -0.1 to -0.3 for teenagers aged 16–19, and somewhat smaller elasticities for 20–24-year-olds (Brown et al. 1982; and Brown 1988).

In contrast, the data on state minimum wage laws and state-level economic conditions permit a pooled time-series cross-section analysis of minimum wage effects, exploiting the greater variation in relative minimum wages at the state level, and avoiding the measurement error caused by using a uniform federal minimum wage. Moreover, because the variation in minimum wage levels across states should be more independent of the growth of social welfare and training programs than are changes in the federal minimum, this analysis should yield more reliable estimates of the effects of minimum wages on employment and unemployment of teenagers and young adults.

Specifically, our panel data set permits us to estimate an equation of the form

$$(2) \quad E_{it} = \alpha_0 + \alpha_1 \mathrm{MW}_{it} + X_{it}\beta + Y_t\gamma + S_i\delta + \epsilon_{it},$$

where i indexes states and t indexes years. Y_t is a set of fixed year effects, and S_i is a set of fixed state effects. Throughout, we assume that ϵ_{it} is serially uncorrelated and orthogonal to the regressors, but that Y_t and S_i may be correlated with the other regressors. For the analysis, 751 observations are available (data for 50 states and Washington, D.C. multiplied by the 13 years for which complete data are available, plus an additional four years of data for the 22 larger states identified in the CPS from 1973 through 1976).

The inclusion of state effects addresses a major criticism of studies that use purely cross-sectional data: that unmeasured economic conditions of state economies might give rise to persistently tight labor markets and high wages in particular states (Freeman 1982). In equation (2), such a relationship would generate a negative correlation between MW_{it} and E_{it}, and hence an estimate of the disemployment effect that is too large. Panel data can resolve this problem, while still exploiting geographic variation. We can also control for year effects in the data, corresponding perhaps to business-cycle or cohort-size effects that are not captured in the variables usually included in minimum wage studies; this problem is insurmountable in a time-series study.[10]

An important specification issue with

ratio, rather than the unemployment rate, because, as pointed out by Mincer (1976), the effects of minimum wages on unemployment rates are ambiguous

[9] As Brown et al. (1982) point out, a simple supply and demand model with homogeneous workers implies that in the presence of a (binding) minimum wage, employment is demand-determined. Supply variables become important because many workers, including teenagers, earn more than the minimum wage Thus, overall employment will depend on supply as well as demand variables

[10] Only three studies for the United States and one for Canada use repeated observations on states (or regions) to remove the influence of state (or region) effects on cross-sectional estimates For the United States, Cunningham (1981) and Cogan (1981) use states as the unit of observation, and attempt to identify minimum wage effects from changes in employment rates across decennial Censuses, neither of these studies uses information on youth or student subminimums, and only the Cunningham paper uses data on state minimum wage levels. Lester (1946) provides a similar experiment, comparing employment growth in particular industries in the North and the South following the implementation of the FLSA, which had a larger impact in the lower-wage South. Swidinsky (1980), studying minimum wage effects in Canada, takes an approach most similar to ours, using data for five regions over a 20-year period. He reports an employment elasticity of -0.17 for Canadian teenagers. Swidinsky also notes that youth and student subminimum wages vary across regions, but does not carry out a direct analysis of the moderating effects of subminimum wages.

respect to equation (2) is whether to include the proportion of the age group enrolled in school as an independent variable. The reason for including school enrollment is to capture exogenous variation in schooling patterns, due, for example, to changes in the costs of or returns to education or in mandatory schooling laws. Omitting the enrollment rate can lead to specification error, because enrollment rates and minimum wages may be (partially) correlated. *A priori*, the sign of this correlation is ambiguous. High minimum wages may lead young persons to stay in school, either because of worsened employment opportunities or because schooling increases the probability of employment in the covered sector (Leighton and Mincer 1981). Alternatively, if being in school makes job search more difficult, high minimum wages may reduce enrollment as young persons leave school to queue for minimum wage jobs (Mincer 1976).

On the other hand, including the school enrollment rate (without instrumenting) may lead to endogeneity bias. Because school and work represent alternative opportunities for many young persons, exogenous changes in employment rates may affect enrollment rates as well, imparting a negative bias to the coefficient of the school enrollment variable (if factors associated with high employment rates lead to low enrollment rates). The bias transmitted to the coefficient of the minimum wage variable again is ambiguous *a priori*, depending on the (partial) correlation between the enrollment rate and the minimum wage variable; a negative correlation will lead to an overly large estimated minimum wage effect.

The bias from the specification error and the endogeneity bias, however, will be of opposite signs, suggesting that estimates from specifications alternately including and excluding the enrollment rate will bracket the true minimum wage effect. One potential solution, of course, is to include the school enrollment rate and instrument for it. Unfortunately, we do not believe that there are valid exclusion restrictions on the basis of which to instrument for the enrollment variable. Instead, throughout the paper we report least squares results both excluding and including the enrollment rate, and present additional evidence on the likelihood of bias in each specification.

Table 2 reports results from alternative specifications of the pooled time-series cross-section standard minimum wage model. Panel A reports specifications with different combinations of fixed state and year effects for teenagers (aged 16–19), and B repeats that analysis for young adults (16–24). Unless otherwise noted, the standard within-group estimator is used to estimate the fixed-effects models.

Columns (1) and (2) report estimates from specifications using fixed state and year effects. In the specifications excluding the school enrollment rate, the estimated minimum wage effect is slightly positive for teenagers and is negative for young adults, and is marginally significant only for young adults. In the specifications including the school enrollment rate, the disemployment effects are negative and statistically significant for both teenagers and young adults, with a larger disemployment effect for teenagers. The corresponding elasticities of the employment-to-population ratio with respect to the minimum wage variable, evaluated at the sample means, are reported in the last row of each table.

The remaining columns of Table 2 report estimates from specifications excluding year effects, excluding state effects, and excluding both. The estimated minimum wage effects vary considerably as different fixed effects are excluded. The elasticities are more positive when year effects are dropped (as compared to columns (1) and (2)) and more negative when state effects are dropped. The differences between the estimated elasticities with and without the fixed state effects confirm the suspicions of critics of earlier cross-section studies. Omitting state effects imparts a negative bias to the minimum wage estimates, suggesting that unmeasured local economic conditions can com-

Table 2. Within-Group and OLS Estimates of Minimum Wage Effects on the Employment-to-Population Ratio, Teenagers (16–19) and Young Adults (16–24).[a]
(Standard Errors in Parentheses)

Independent Variable	Specification (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Teenagers				
Minimum Wage	07 (10)	– 17 (07)	21 (07)	–.11 (05)	– 33 (08)	– 25 (04)	– 18 (07)	– 23 (04)
Proportion of Population in Age Group	–.19 (22)	– 11 (15)	–.24 (15)	– 13 (10)	.35 (28)	– 05 (.15)	.02 (.21)	– 16 (11)
Prime-Age Male Unemployment Rate	– 54 (11)	– 31 (07)	– 86 (08)	–.63 (06)	–1 53 (13)	– 76 (07)	–1 39 (11)	–.80 (.06)
Proportion of Age Group in School	—	– 75 (03)	—	–.75 (03)	—	–.95 (02)	—	– 96 (02)
Year Effects	Y	Y	N	N	Y	Y	N	N
State Effects	Y	Y	Y	Y	N	N	N	N
p-value for Restricted Model[b]	—	—	00	00	00	00	00	.00
$\bar{R}^2$	.69	86	68	85	20	.78	18	78
Elasticity[c]	06	– 14	17	–.09	– 27	– 20	– 14	– 18
				B. Young Adults[d]				
Minimum Wage	–.11 (07)	– 16 (06)	02 (06)	– 12 (04)	–.21 (06)	– 23 (04)	– 14 (05)	– 21 (04)
Proportion of Population in Age Group	42 (10)	08 (07)	– 19 (06)	– 24 (.05)	.28 (11)	– 16 (08)	– 11 (08)	– 27 (05)
Prime-Age Male Unemployment Rate	– 53 (.08)	– 47 (06)	– 70 (06)	– 69 (05)	–1 36 (09)	–1 13 (07)	–1 17 (08)	–1 06 (.05)
Proportion of Age Group in School	—	– 80 (04)	—	– 82 (04)	—	–.99 (03)	—	–1.02 (03)
p-value for Restricted Model[b]	—	—	00	00	00	00	00	00
$\bar{R}^2$	.70	82	66	80	28	66	23	65
Elasticity[c]	– 07	– 10	01	– 07	– 13	– 14	– 09	– 13

[a] The sample consists of 751 observations covering the 50 states and Washington, D.C For the 22 states identified in the CPS as early as 1973, the sample covers the years 1973–89; for the remaining states it covers the period 1977–89 The minimum wage variable is the greater of the state or federal minimum wage level, multiplied by federal minimum wage coverage for the state, divided by the average wage in the state The prime-age male unemployment rate is for men aged 25–64.
[b] Likelihood-ratio test
[c] Evaluated at sample means
[d] The inclusion/exclusion of state and year effects for young adults parallels that for teenagers

plicate the estimation of minimum wage effects in such studies. The other coefficients also change when fixed effects are excluded, most noticeably the estimated coefficient on the prime-age male unemployment rate. These changes in coefficients, along with the rejection of the restrictions imposed by dropping either the state or year effects, lead us to select as the best specifications those with fixed state and year effects.[11]

[11] In results not reported in the tables, we checked the validity of the linear specification of our fixed year- and state-effects specification. We set up a grid search over a specification of the model with a Box-Cox transformation of the dependent and

More generally, minimum wage elasticities from our fixed-effects models that include the school enrollment variable are broadly consistent with the evidence presented by Brown et al. (1982), if perhaps toward the lower end of their consensus range. However, the positive estimated minimum wage elasticity for teenagers from the fixed-effects model that excludes the school enrollment rate is unusual.[12] A critical question, then, is whether the change in the minimum wage effect for teenagers in these alternative specifications reflects specification error in the equation excluding this rate, or instead reflects bias in the column (2) estimates stemming from endogeneity of the enrollment rate. For two reasons, we believe that the specification error that results from omitting the school enrollment rate is more severe than the endogeneity bias that results from including the enrollment rate.

First, one might expect minimum wage effects, if they are present, to be stronger for teenagers than for young adults, since more teenagers earn wages near the minimum. This expectation is confirmed only in the specifications including the enrollment rate; in the specifications excluding the enrollment rate, the estimated employment effects are negative only for young adults. The same general result appears in later tables; in specifications excluding the school enrollment rate, minimum wage effects are weaker (although often negative) for teenagers than for young adults, whereas the reverse holds for specifications including the enrollment rate.

Second, the potential endogeneity bias arises from an auxiliary equation in which the enrollment rate is a function of, among other variables, the employment rate. Factors that increase the employment rate through ϵ_{it} in equation (2) then reduce the enrollment rate in the auxiliary equation, leading to an overly strong negative estimated correlation between employment and enrollment rates in equation (2). Presumably, one important factor that shifts the employment rate is the level of economic activity. If so, then we would expect the endogeneity bias in the coefficient of the enrollment rate, in equation (2), to be much more severe if the prime-age male unemployment rate is excluded from the equation. But when the unemployment rate is excluded from the specifications shown in Table 2, the estimated coefficient of the enrollment rate is virtually the same as in the specifications including the unemployment rate; the coefficient differs by at most 0.02 for the specifications including year and state effects. Because the inclusion of the unemployment rate should remove a significant portion of the endogeneity bias, these results suggest that endogeneity bias is relatively unimportant. This conclusion leads us to prefer the specifications including the school enrollment rate as providing more nearly unbiased estimates of minimum wage effects.[13]

independent variables, with the same transformation applied to all variables; the Box-Cox parameter ranged from 0 to 1 in increments of 0.1 For all four alternatives (teenagers and young adults, with and without the school enrollment rate), the likelihood was monotonically increasing as the Box-Cox parameter increased This result validates the linear specification, and in particular rules out a double-log specification. (A double-log specification seems dubious on *a priori* grounds, since it implies that an increase in the minimum wage from 50 cents to one dollar has the same percentage effect on employment as an increase from two dollars to four dollars.)

[12] The increase in the estimated disemployment effects of minimum wages for teenagers, when school enrollment rates are added to the equation, implies that the partial correlation between the school enrollment rate and the minimum wage variable is negative for this group. In specifications identical to those in column (1) of Table 2 except that the enrollment rate is the dependent variable, the estimated coefficient (standard error) of the minimum wage variable was −0.32 (0 10) for teenagers and −0 07 (0 06) for young adults In "Do Minimum Wages Reduce Employment?" (this issue), Card finds a similar result for California: the school enrollment rate of teenagers fell with the rise in the minimum wage. In contrast, Mattila (1978) finds that, in time-series data, minimum wages are positively associated with enrollment rates

[13] We should point out, however, that the implied estimate of the effect of the enrollment rate on the employment rate (in the range −0.75 to −0.82) is large compared with the mean difference in employ-

Other potential biases. Another possible source of bias arises from the potential endogeneity of state minimum wage levels. If state legislators choose the timing of minimum wage increases to correspond to periods in which minimum wage increases will have the smallest (or perhaps least noticed) disemployment effects, the estimated minimum wage effects in Table 2 will be biased upward, as long as economic conditions are not fully captured in the prime-age male unemployment rate. To test for this type of bias, we consider instrumental-variables estimation of the fixed-effects specifications from Table 2.

Instrumental-variables estimation is straightforward in the presence of fixed effects; the state and year effects simply have to be included in the first-stage regression of the endogenous variable on the exogenous variables and the instruments. Our instrumental variable is the mean of the minimum wage level in all geographically bordering states (adjusted for coverage in the state by the FLSA, and divided by the mean wage in the state). This instrument will be valid if higher minimum wage levels in neighboring states influence the likelihood of enacting a higher minimum wage in a state, but neither affect nor are affected by labor market conditions in the state.[14] On the other hand, minimum wage levels in bordering states may influence a state's labor market conditions through the mobility of firms, in which case the minimum wage in bordering states may belong in the employment equation.

Two-stage least squares estimates of the fixed-effects specification are reported in panel A of Table 3. For both teenagers and young adults, the instrumental-variables estimates of the employment elasticities are more strongly negative, consistent with endogeneity bias generating a positive correlation between employment rates and minimum wages. The standard errors of the estimates, however, are large enough that the coefficient estimates are not significantly different from those that ignored endogeneity, and hence provide no statistical evidence of endogeneity bias. In view of these results, along with concerns about the validity of the instrument, we ignore the endogeneity of minimum wages in the rest of this paper, although this is a potentially promising avenue for future research.

A second potential problem with estimating minimum wage effects from equation (2) is that the denominator of the relative minimum wage variable, which is the average level of wages in the state, may reflect the interaction of labor demand and supply. For example, exogenous labor demand shifts will induce a positive correlation between employment rates and average wages, possibly causing a spurious negative correlation between the minimum wage variable and employment rates, because the average wage appears in the denominator of the minimum wage variable. Of course, because year effects are included in the models estimated, only state-specific variation in labor demand shifts would create this bias. Unfortunately, there do not appear to be valid identifying assumptions to address this problem by instrumenting for the average wage with an exogenous variable from the labor demand function; equation (2) is intended to be the reduced form of the labor demand and supply equations (aside from this problem) and therefore already includes all exogenous variables from these equations.

Nonetheless, we would like to verify that the minimum wage effects estimated in

ment rates between enrolled and non-enrolled youth, for example, for 16–24 year-olds in 1978 (roughly the middle of our sample), the employment rates were 42.5% for the enrolled and 73.3% for the non-enrolled. These differences might be suggestive of downward simultaneity bias in the estimated coefficient of the enrollment rate. Of course, the raw difference in means could easily understate (or overstate) the "causal" effect of the enrollment rate by ignoring the effects of other variables, or by ignoring heterogeneity bias captured by the fixed state effects in Table 2. Nonetheless, these differences point to the value of efforts to address directly the endogeneity of enrollment rates in minimum wage equations, through the construction of valid instruments.

[14] In Maine, for example, the state minimum wage law mandates that the minimum wage cannot exceed the average minimum wage in the other five New England states.

Table 3. Examination of Potential Biases in Within-Group Estimates of Minimum Wage Effects.[a]
(Standard Errors in Parentheses)

Independent Variable	Teenagers (16–19) (1)	Teenagers (16–19) (2)	Young Adults (16–24) (3)	Young Adults (16–24) (4)
	A. Instrumental-Variables Estimates			
Minimum Wage	− 02	−.28	− 33	−.32
	(.20)	(14)	(.15)	(11)
Proportion of Population in Age Group	−.18	−.08	43	.09
	(22)	(15)	(.10)	(.08)
Prime-Age Male Unemployment Rate	− 53	− 31	−.52	− 46
	(.11)	(.07)	(08)	(.06)
Proportion of Age Group in School	—	− 76	—	− 80
		(.03)		(.04)
Elasticity[b]	−.01	− 22	−.21	−.20
	B. Separating Mean Wage from Coverage-Adjusted Minimum Wage			
Coverage-Adjusted Minimum Wage Level	01	− 03	− 02	−.03
	(.02)	(01)	(.02)	(.01)
Mean Wage	−.01	004	.003	.01
	(.01)	(004)	(004)	(.003)
Proportion of Population in Age Group	− 24	− 12	.42	.09
	(.22)	(15)	(10)	(07)
Prime-Age Male Unemployment Rate	− 55	− 33	−.53	− 47
	(11)	(.07)	(08)	(.06)
Proportion of Age Group in School	—	−.75	—	−.80
		(.03)		(04)
$\bar{R}^2$	.69	86	.69	.82
p-value for Restricted Model in Log-Log Specification[c]	.09	08	.71	.70

[a] The sample consists of 751 observations covering the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1973–89, for the remaining states it covers the period 1977–89. All specifications include year effects The instrumental variable in Panel A is the average of the minimum wage level in bordering states, each multiplied by federal minimum wage coverage for the state and divided by the average wage in the state. For Hawaii and Alaska the state's value of the minimum wage level is substituted for the average of the bordering states

[b] Evaluated at sample means

[c] This is the p-value from a likelihood ratio test of the log-log specification of the model. In the unrestricted model the coverage-adjusted minimum wage level and the mean wage are entered separately. In the restricted model the coefficient of the coverage-adjusted minimum wage level is restricted to be the same in absolute value as the coefficient of the mean wage, but with the opposite sign.

Table 2 indeed reflect the negative influence of minimum wages on employment. As a suggestive exercise, in panel B of Table 3 we report results from specifications in which we separate the numerator and denominator of the minimum wage variable. The estimates reveal a negative effect of the coverage-adjusted minimum wage level on employment, for the three fixed year and state effects specifications in which there was a negative effect in Table 2. In contrast, the average wage has a positive effect in the same three specifications. The positive effect of the average wage does not necessarily reflect labor demand shifts, since an increase in average wages should otherwise reduce the disemployment effects of minimum wages. But assuming that whatever bias exists in the coefficient of the mean wage is not transmitted to the coefficient of the coverage-adjusted minimum wage level, these results suggest a true negative effect of minimum wages.

Reconciling Conflicting Evidence in Recent Research on Minimum Wage Effects

Our finding of significant negative effects of minimum wages on the employment of teenagers and young adults contrasts with conclusions drawn by Card in his study of the rise in California's minimum wage between 1987 and 1989 and in his study of the increase in the federal minimum wage in 1990 (papers included in this issue). Specifically, Card reports a positive contemporaneous correlation between changes (that is, short first differences) in the minimum wage and changes in the employment of teenagers.[15] To determine whether Card's results hold more generally, we examine first-difference estimates of our four specifications. We use pairwise first differences, forming differences over a year and including the data only for the odd-numbered years, to avoid the serial correlation in the errors that would be induced by using the data for all years.[16] The results are presented in panel A of Table 4.

The estimated minimum wage effects are strikingly different from the fixed-state-effects estimates reported in Table 2, and are similar to the results reported by Card. In particular, in three of the four specifications there is a positive (although statistically insignificant) effect of minimum wages on the employment-to-population ratio, and there is clearly no statistically significant evidence of negative effects of minimum wages on employment. Thus, a principal source of the differences between Card's results and those reported here is the method of estimation of equation (2)—specifically, its estimation using the within-group (least squares dummy variable) estimator, as in Table 2, or instead using a short first-difference estimator, as in Card's papers and in panel A of Table 4. Because both estimators of equation (2) should be consistent, the differences in the results suggest that the equation is misspecified. In this section, we consider two sources of model misspecification that could underlie these differences.

Model misspecification: the fixed-effects assumption. The estimates in Table 2 suggest that omitting fixed state effects leads to overstated disemployment effects of minimum wages; that is, the coefficients of the minimum wage variable are biased downward. One possible source of misspecification in equation (2) is an omitted variable related to state economic conditions that is not completely fixed, but rather is serially correlated. If the omitted state effect is positively serially correlated, then the short first-difference estimator should net out the state effect more completely than the within-group estimator, which uses longer differences, on average. In this case, the within-group estimator would still be prone to downward bias from the omitted state effect; in contrast, the short first-difference estimator would yield more nearly consistent estimates of the minimum wage effects.

To explore this possibility, we conduct a specification test for the fixed-effects assumption (Heckman and Hotz 1989). The test entails computing a first-difference estimate, including in the specification the level of the dependent variable from a period prior to that over which the first difference is computed. If the earlier level of the dependent variable enters the first-differenced equation with a statistically significant coefficient, an omitted state effect likely remains in the differ-

[15] The samples used in Card's papers differ from ours one focuses on a comparison of California's labor market with a few other labor markets over the period 1987–89, the other looks at the 50 states and Washington, D.C , as does our paper, but only over the period 1989–90.

[16] Correcting for serial correlation in fixed-effects models is complicated because of bias in the standard quasi-difference estimator of the correlation parameter (see, for example, Kiefer 1980). The results that follow are unchanged if even-numbered years are used Odd-numbered years were chosen in order to utilize the data for 1989, when the maximum number of states had minimum wages exceeding the federal level

enced data, and the fixed-effects assumption becomes questionable.[17]

To examine this question, we computed first-difference estimates of equation (2) over an eight-year interval, for all of the years in our data set for which this computation is possible. This is the length of the longest difference used in the within-group estimator (although in the within-group estimator, these differences are defined relative to the state-specific mean); if the fixed-effects assumption fails, it should be most apparent in the long difference. Panel B of Table 4 reports these estimates. Consistent with the possibility of a downward bias associated with time-varying state effects, the estimated minimum wage coefficients in the long first difference are strongly negative.

Results from the specification tests, however, reported in panel C of the table, indicate that the long-difference specification does not violate the fixed-effects assumption particularly strongly. The estimated coefficient from the employment-to-population ratio lagged nine years, when it is added to the eight-year first-difference specification, is statistically significant in two of the four specifications. But evidence against the fixed-effects assumption is stronger for the short first difference. When the employment-to-population ratio lagged two years is added to the one-year first-difference specification for which estimates were reported in Panel A of Table 4, the coefficient of the lagged dependent variable is statistically significant and negative in three of the four specifications.[18] Based on this evidence, we conclude that an inappropriate fixed-effects assumption in the long-difference or within-group estimates does not underlie the differences between these estimates and the short first-difference estimates.

Model misspecification: dynamic effects. An alternative explanation of the differences between the within-group and short first-difference estimates is that the basic model is misspecified because it ignores lags in the effects of minimum wage changes.[19] If equation (2) is correctly specified, then both the within-group and short (or long) first-difference estimators are consistent. If the equation is misspecified by omitting lags, however, then the omitted variable bias is likely far more severe in the short first-difference estimator.

Suppose that the true model is not equation (2), but rather

$$(3)\quad E_{it} = \beta MW_{it} - \gamma MW_{it-1} + S_i\delta + \epsilon_{it},$$

where $\gamma > 0$, and other regressors from equation (2) have been suppressed for notational convenience. Suppose that the estimated model is

$$(4)\quad E_{it} = \beta' MW_{it} + S_i\delta' + \epsilon_{it}.$$

That is, the lagged minimum wage variable is omitted. What is the effect of the omitted variable bias on the estimate of

[17] Suppose the fixed state effect, S, is AR1,

$$S_{it} = \rho S_{it-1} + \epsilon_{it},$$

where ϵ is i.i.d., and uncorrelated with S and the other regressors, and ρ is less than one in absolute value. Then in a first difference computed over τ years, the state effect that remains is $(\rho^{\tau} - 1)S_{it-\tau}$. For $\rho > 0$, this state effect is likely to be closer to zero the shorter is τ, and to become a larger negative number as τ grows. Thus, in this example, the short first-difference estimator will be less prone to bias from the omitted state effect. Since $S_{it-\tau}$ is positively correlated with $E_{it-\tau-1}$, including this latter variable will detect failure of the fixed-effects assumption. Finally, we might expect the coefficient on $E_{it-\tau-1}$ to be negative, since $S_{it-\tau}$ enters the differenced equation with a negative coefficient.

[18] We also computed the specification test for four- and six-year first differences, to see if there was anything idiosyncratic about the eight-year first difference. For the six-year first difference, the fixed-effects assumption is rejected for two specifications, and for the four-year first difference, it is rejected only once.

[19] Brown et al. (1982) discuss the arguments for and against the likely existence of significant lags in minimum wage effects. Lagged effects may arise for the standard reasons, either because of hiring and training costs, or because of an inability to adjust other inputs quickly. But strong lags in minimum wage effects are sometimes considered less likely because of high turnover among low-wage workers, and because minimum wage changes are typically enacted some time before they actually take effect. Card, in "Using Regional Variation in Wages . . ." (this issue), reports empirical evidence consistent with a lag between legislated increases in minimum wages and upward wage adjustments by employers.

Table 4. First-Difference Estimates of Minimum Wage Effects on the Employment-to-Population Ratio, and First-Difference Specification Tests.[a]
(Standard Errors in Parentheses)

Independent Variable	*Teenagers (16–19)*		*Young Adults (16–24)*	
	(1)	*(2)*	*(3)*	*(4)*
	A. Short (One-Year) First Difference			
Minimum Wage	25	.07	08	−.02
	(17)	(12)	(12)	(10)
Proportion of Population in Age Group	45	− 20	12	− 11
	(34)	(26)	(15)	(13)
Prime-Age Male Unemployment Rate	− 17	− 15	− 15	− 20
	(.17)	(12)	(12)	(10)
Proportion of Age Group in School	—	− 77	—	− 73
		(05)		(06)
$\bar{R}^2$	01	46	01	28
Elasticity[b]	.20	06	05	− 01
	B. Long (Eight-Year) First Difference			
Minimum Wage	− 01	− 31	− 20	− 23
	(14)	(09)	(.10)	(08)
Proportion of Population in Age Group	−.05	31	.51	13
	(34)	(22)	(14)	(10)
Prime-Age Male Unemployment Rate	− 78	− 45	− 57	− 50
	(15)	(10)	(11)	(08)
Proportion of Age Group in School	—	− 81	—	−.88
		(04)		(.06)
$\bar{R}^2$	11	.62	17	.56
Elasticity[b]	− 01	−.24	− 12	− 14
	C. Specification Tests			
Employment-to-Population Ratio				
Nine-Year Lag, in Panel B Specification	−.06	− 04	.02	01
	(03)	(02)	(02)	(.02)
Two-Year Lag, in Panel A Specification	− 05	− 04	− 05	−.04
	(03)	(02)	(.02)	(.02)

[a] In Panel A, the sample consists of 350 observations covering the 50 states and Washington, D C For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974–89, for the remaining states it covers the period 1978–89 Only odd-numbered years were included, to avoid serial correlation introduced by first differencing

In Panel B, the sample consists of 292 observations. For the 22 early CPS states, the sample covers the years 1982–89, for the remaining states it covers the period 1986–89 Because there were no overlapping differences, using all observations in these periods does not induce serial correlation in the errors All specifications include year effects

[b] Evaluated at sample means

β'? In the within-group estimation, we have

(5) $$E(b') = \beta - \gamma\beta_x^{WG},$$

where b' is the OLS estimate of β', and β_x^{WG} is the coefficient from the auxiliary regression

(6) $$MW_{it-1} = \beta_x^{WG} MW_{it} + S_t\delta + \eta_{it}.$$

In the first-difference estimation, we have

(7) $$E(b') = \beta - \gamma\beta_x^{FD},$$

where β_x^{FD} is the coefficient from the auxiliary regression

(8) $$\Delta MW_{it-1} = \beta_x^{FD} \Delta MW_{it} + \Delta\eta_{it},$$

which is just the first-difference form of equation (6).

Equation (6) is not a structural equation, since it seems likely that MW_{it} is correlated with the residual (assuming that there is some serial correlation in MW_{it}). All we are interested in, however, is comparing the relative magnitudes of the additional bias from the alternative methods of treating the fixed state effects, so we can interpret η_{it} in equation (6) as the part of the residual that is uncorrelated with MW_{it}. The only reason for the bias to differ between the fixed-effects and first-difference estimators is that β_x^{WG} and β_x^{FD} differ. Again, though, we are simply comparing the coefficients from a within-group and first-difference estimator of the same regression, so why should these coefficients differ?

The reason is that in the first-difference estimator, the construction of the differences induces a strong negative correlation between the omitted lagged minimum wage variable and the included contemporaneous minimum wage variable. To develop this argument explicitly, note that equations (6) and (8) suffer from the same problem as the standard dynamic model with fixed effects (Kiefer 1980), namely, the problem that estimation induces a correlation between the residuals and the regressors. This is easiest to see in equation (8). The right-hand-side variable in equation (8) is $(MW_{it} - MW_{it-1})$, and the residual is $(\eta_{it} - \eta_{it-1})$. But η_{it} and MW_{it-1} are obviously correlated; the covariance between the regressor and the residual is $-\mathrm{Cov}(MW_{it-1}, \eta_{it}) = -\mathrm{Var}(\eta_{it})$. If this negative bias is large enough to bring about a negative β_x^{FD} in equation (8), then equation (7) implies that the first-difference estimator of β' (the coefficient on MW_{it}) in equation (4) is upward biased.

When the within-group estimator is used, however, the bias is less by a factor of T (the number of time periods). The within-group estimation is carried out by subtracting from each variable its state-specific mean and using OLS on the transformed data. Thus, β_x^{WG} in equation (6) is estimated from the regression

$$(9)\quad (MW_{it-1} - T^{-1}\Sigma MW_{it-1}) = \beta_x^{WG}(MW_{it} - T^{-1}\Sigma MW_{it}) + (\eta_{it} - T^{-1}\Sigma \eta_{it}).$$

There is still a correlation between the error and the regressor, since η_{it} and MW_{it-1} still appear in them, but the covariance between the error and regressor is now $-[(T+1)/T^2]\mathrm{Var}(\eta_{it})$,[20] which converges to $-T^{-1}\mathrm{Var}(\eta_{it})$ as T gets large.[21] Thus, the within-group estimator of the coefficient β' in equation (4), given that equation (3) is the true model, is less biased by a factor of T than the first-difference estimator. If this bias is small enough so that the estimate of β_x^{WG} in equation (6) remains positive, then equation (5) implies that the within-group estimator of β' in equation (4) is downward biased, in contrast to the first-difference estimator.

Thus, the existence of lagged negative minimum wage effects could in principle account for the difference between the within-group and first-difference estimates. To examine this possibility, in Table 5 we report estimates of dynamic specifications of minimum wage effects using both within-group and first-difference estimators. The specifications for which coefficient estimates are reported include the contemporaneous minimum wage variable as well as the variable lagged once. The estimates of the minimum wage coefficients reveal evidence of significant lags in these effects. In all eight columns, the coefficient on the lagged minimum wage variable is more negative than that on the contemporaneous minimum wage variable, and in the young-adult specifications the lagged coefficient is statistically significant. In addition, the long-run elasticities from the fixed-effects and first-difference estimates are considerably closer. The marked changes in the first-difference estimates of the employment elasticities, coupled with

[20] This expression is true for all but the first or last observations, for which the covariance is.

$$-[1/T^2]\mathrm{Var}(\eta_{it})$$

[21] This follows because for any t,

$$\mathrm{Cov}(MW_{it} - T^{-1}\Sigma MW_{it}, \eta_{it} - T^{-1}\Sigma \eta_{it}) = -T^{-1}\mathrm{Cov}(MW_{it}, \eta_{it+1}) - T^{-1}\mathrm{Cov}(MW_{it-1}, \eta_{it}) + T^{-2}\sum_{t=1}^{T-1}\mathrm{Cov}(MW_{it-1}, \eta_{it}), = -[(T+1)/T^2]\mathrm{Var}(\eta_{it})$$

Table 5. Withın-Group and Fırst-Difference Estımates of Dynamic Specifications of Minimum Wage Effects on the Employment-to-Populatıon Ratıo.[a]
(Standard Errors in Parentheses)

Independent Varıable	*Teenagers (16–19)* (1)	(2)	(3)	(4)	*Young Adults (16–24)* (5)	(6)	(7)	(8)
Minımum Wage	10	− 12	15	01	− 03	− 10	−.01	− 09
	(.11)	(08)	(17)	(.13)	(08)	(06)	(.13)	(11)
Mınimum Wage, Lagged One Year	−.14	− 12	− 30	− 19	−.26	−.18	− 26	− 22
	(.12)	(07)	(18)	(13)	(08)	(06)	(13)	(11)
Proportıon of Populatıon ın Age Group	− 11	− 13	46	− 19	42	08	14	− 09
	(23)	(.16)	(34)	(26)	(10)	(08)	(15)	(13)
Prıme-Age Male Unemployment Rate	−.53	−.29	− 10	− 11	−.52	−.47	−.09	− 15
	(.11)	(08)	(17)	(13)	(09)	(06)	(12)	(10)
Proportıon of Age Group ın School	—	− 77	—	−.77	—	−.81	—	− 73
		(.03)		(05)		(04)		(06)
Estimator[b]	WG	WG	FD	FD	WG	WG	FD	FD
p-values [c] 0–1 Lags vs. 0 Lags	20	12	.09	14	00	00	04	.04
$\overline{R}^2$	70	86	.01	46	71	83	.02	.29
Elastıcity[d]	−.03	−.19	−.12	−.15	− 18	− 17	− 17	−.19
	(.10)	(07)	(.24)	(17)	(.06)	(04)	(.13)	(11)
Elastıcıtıes from Alternatıve Specıfıcatıons								
0 Lags	04	− 14	.20	.06	− 08	− 11	.05	− 01
1 Lag Only	− 08	− 14	−.28	−.16	− 17	−.14	−.16	− 12
0–1 Lags of All Variables in Equatıon	01	− 17	− 06	−.17	− 15	− 15	−.15	− 22

[a] The sample covers the 50 states and Washington, D.C For the 22 states ıdentified in the CPS as early as 1973, the sample covers the years 1974–89 for the fixed-effects estımates, and 1975–89 for the first-dıfference estımates, for the remaining states the correspondıng perıods are 1978–89 and 1979–89. For the first-dıfference estimates, the sample ıncludes only odd-numbered years, to avoid serial correlation ıntroduced by first dıfferencıng

[b] All specıficatıons ınclude year effects.

[c] Lıkelıhood-ratio test.

[d] Long-run, evaluated at sample means Standard errors treat coefficient estımates, but not means, as random.

evidence of significant lagged minimum wage effects, are consistent with greater upward bias in the first-difference estimates than in the within-group estimates in specifications omitting the lags.[22]

In addition, we computed the auxiliary regressions (6) and (8), and verified that downward bias dominates the first-difference estimates (equation (8)), but not the within-group estimates (equation (6)). For the four alternative specifications the coefficients for equation (8) were between −0.26 and −0.27. In contrast, the coefficients were between 0.42 and 0.43 for equation (6). These coefficients are consistent with an upward bias in the first-difference estimates of minimum wage effects in specifications omitting a lagged minimum wage variable and a downward bias in the within-group estimates of minimum wage effects in similar specifications; a comparison of the estimates in Tables 2 and 4 with those in Table 5 reveals exactly these biases.[23] The conclu-

[22] In results reported in Table 6, the same qualıtative conclusıons were drawn from specıfications ıncluding two lags of the minimum wage variable

[23] For example, in Table 2, column (1), panel A,

sion that the omission of lagged minimum wage effects biases the short first-difference estimates upward is buttressed by the finding that the long (eight-year) first-difference estimates in Table 4 indicate strong disemployment effects of minimum wages. These long-difference estimates are not prone to the same bias from omitting lagged minimum wage effects, since the omitted varible (MW_{it-1} − MW_{it-9}) is not necessarily negatively correlated with the included variable ($MW_{it} - MW_{it-8}$).[24]

The table also reports results of specification tests and robustness checks for these dynamic specifications. The p-values from likelihood-ratio tests indicate that the data reject the specification with the contemporaneous minimum wage variable alone for all of the young-adult models. For teenagers, the p-values range from 0.09 to 0.20, a result that does not indicate rejection. But in results not reported in the table, we compared the likelihoods for the non-nested models including only the contemporaneous minimum wage variable, or, alternatively, only the lagged variable. The likelihood was higher for the model with lags in the three cases in which the contemporaneous specification indicated a positive elasticity (the fixed-effects specification excluding school enrollment, and the two first-difference specifications). At the bottom of the table we report elasticities for the specification including only the contemporaneous minimum wage variable and, alternatively, only the lagged minimum wage variable.[25]

These elasticities tell much the same story: once lagged effects are allowed, negative elasticities result for all specifications. In the last row, we report the elasticities including one lag of each of the control variables, to examine whether the lagged minimum wage variable is simply picking up lagged effects of these other variables. The results are largely unchanged.

Finally, the estimates of the elasticities are larger (more negative) than in any of the previous fixed-effects (or first-difference) estimates. The elasticities are bunched in a range from −0.15 to −0.2; the single exception is the specification for teenagers excluding the school enrollment rate. These estimates are close to the midrange of the consensus of past time-series studies.

Robustness checks. In Table 6 we report the elasticities from within-group estimates of the dynamic specification of the model (with one lag of the minimum wage variable), exploring the robustness of the results to variations in data construction, model specification, and sample definition. In panel A we report estimates using no coverage adjustment; the minimum wage variable is simply the minimum wage level divided by the mean wage in the state. The estimated elasticities are generally slightly smaller in absolute value without the coverage adjustment, compared to the elasticities in Table 5 (columns (1), (2), (5), and (6)), probably because a one-unit increase in the coverage-adjusted minimum wage variable corresponds to a larger increase in the minimum wage level than does a one-unit increase in the unadjusted minimum wage variable.

In panel B, we attempt to adjust for coverage by using state minimum wage laws along with the FLSA provisions.[26] In

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the coefficient of the contemporaneous minimum wage variable is 0.07, which rises to 0.10 in Table 5, column (1). In contrast, in Table 4, column (1), this coefficient is 0.25, which falls to 0.15 in Table 5, column (3) The same pattern holds for all of the specifications.

[24] As a further test, we also computed elasticities for first differences of other lengths, using the same sample used for the eight-year long difference reported in Table 4. The elasticities were negative for six- and four-year first differences (except for the specification for teenagers excluding the enrollment rate), and were positive for two- and one-year first differences.

[25] The elasticities from the contemporaneous specification differ slightly from those reported in Table 2 because here they are computed from the same sample for which the lagged model can be estimated

[26] We used the 1974 estimates from the Department of Labor for 1973, and interpolated the 1975 and 1977 estimates for 1976. Estimates beyond 1977 were extrapolated as constant percentages equal to the value in 1977 for subsequent years. Prior to 1976, most state and local government employees were covered by the FLSA as a result of the 1966 and 1974

this case, evidence of negative employment elasticities is weaker. As discussed above, however, we have very little hard evidence on coverage by state minimum wage laws, and therefore discount these estimates.

In panel C, we substitute the federal minimum wage level for the state level, to examine the extent to which our use of state minimum wage levels, in contrast to the federal minimum wage level used in time-series studies, influences the results. The estimated elasticities are slightly larger than those using the state minimum wage levels. In panel D we re-estimate the same models using state population estimates from the CPS to weight the observations.[27] Again, there is relatively little change in the estimates, although there is some widening of the range of elasticities. In panels E and F we report the elasticities that result from including a second lag of the minimum wage variable, and then also introducing one and two lags of each of the control variables. Again, the estimates are little changed.

Finally, in panels G–J we report estimates first splitting the sample into separate time periods, and then splitting the sample between states for which the minimum wage never exceeded the federal level and states that had higher state minimums at some point in the sample period. The estimates reveal stronger elasticities in the latter half of the sample period, and for the states with higher state minimums; these results are mutually consistent, because it was in the latter part of the sample period that relatively more states raised their minimum wages above the federal level.

Evidence on Student and Youth Subminimum Wage Provisions

The final issue we explore in this paper is the potential for youth or student subminimum wage provisions to reduce the adverse disemployment effects of minimum wages. Katz and Krueger (this issue) attempt to assess the likely impact of the new federal subminimum through a small survey of employers in the period immediately following the implementation of the new federal legislation, studying in particular the extent of usage of the federal subminimum in the Texas fast food industry. Because many states have had such subminimums in the past, however, our state-level data provide a complementary means of estimating the impact of these subminimums.

The empirical question is whether—controlling for the state minimum wage level as well as state labor market condition—states with subminimum wages exhibit higher employment rates. The simplest approach to this question is to augment equation (2) to be

$$(10)\quad E_{it} = \alpha_0 + \alpha_1 MW_{it} + \alpha_2 SUB_{it} \cdot (MW_{it} - SMW_{it}) + X_{it}\beta + S_i\delta + \epsilon_{it},$$

where SUB is a dummy variable for the

amendments to the Act In 1976, however, the Supreme Court ruled that the minimum wage and maximum hours provisions of the FLSA could not be applied to state and local government activities that are an integral part of traditional government functions, a ruling that was reversed in 1985 As a result, federal coverage was depressed by the absence of state and local workers from 1977 to 1984, but picked up these workers again in 1985 Consistent with the treatment of state and local government workers in the federal coverage estimates, for states that explicitly covered these workers we assumed that state coverage was equal to its 1977 value from 1978 through 1984, and its 1976 value (interpolated from 1975 and 1977 Department of Labor estimates) from 1985 through 1989 For other states, we simply used the 1977 value throughout the sample period Colorado and Kansas did not enact a state minimum wage until 1978, after the last available estimate of state coverage, we use state coverage estimates of zero for these states In cases for which the sum of the federal coverage estimate and the state coverage estimate exceeded unity, we adjusted the state coverage estimate downward to constrain the two to sum to unity

Given our coverage estimates, the numerator of the relative minimum wage variable is defined as follows: for observations with minimum wage levels greater than or equal to the federal level, the state level multiplied by the sum of federal and state coverage; for observations with a minimum wage below the federal level, the federal minimum wage multiplied by federal coverage, plus the state minimum wage multiplied by state coverage

[27] One reason not to weight the estimates throughout the paper is that a larger state does not necessarily provide more accurate estimates for a single labor market, because a larger state is more likely to encompass many labor markets

Table 6. Sensitivity of Within-Group Estimates of Dynamic Specifications of Minimum Wage Effects on the Employment-to-Population Ratio: Long-run Elasticities Evaluated at Sample Means.[a]
(Standard Errors in Parentheses)

	Teenagers (16–19)		*Young Adults (16–24)*	
Change in Specification	*Excluding Enrollment (1)*	*Including Enrollment (2)*	*Excluding Enrollment (3)*	*Including Enrollment (4)*
A. No Coverage Adjustment (N = 700)	−.06 (.11)	− 13 (08)	−.09 (06)	− 11 (.05)
B. Federal and State Coverage Adjustment (N = 700)	.11 (.09)	− 02 (.06)	−.07 (.05)	−.09 (04)
C. Federal Minimum Wage Level in Place of State Minimum Wage Level (N = 700)	−.07 (.10)	−.20 (.07)	− 20 (.06)	−.17 (04)
D. Weighting by State Population (N = 700)[b]	06 (.11)	− 18 (.07)	−.07 (06)	−.12 (.04)
E 0–2 Lags of Minimum Wage Variable (N = 649)	.02 (.12)	− 21 (.08)	− 22 (.07)	−.21 (.05)
F 0–2 Lags of All Variables (N = 649)	.06 (.12)	− 21 (.08)	−.18 (06)	−.19 (.05)
G. Sample Restricted to 1974–81 (N = 321)	−.18 (.15)	−.10 (10)	−.20 (.09)	− 09 (07)
H. Sample Restricted to 1982–89 (N = 408)	− 03 (.15)	−.11 (.10)	− 15 (08)	− 17 (.06)
I Sample Restricted to States with Minimum Wage Level Equal to Federal Minimum Wage in All Years (N = 488)	−.01 (14)	−.16 (.10)	−.09 (08)	−.11 (.06)
J Sample Restricted to States with Minimum Wage Level Above Federal Minimum Wage in at Least One Year (N = 212)	−.01 (.15)	− 27 (09)	−.18 (08)	−.22 (06)

[a] The sample covers the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974–89, and for the remaining states it covers the period 1978–89, for rows A–D, and for rows G–J, with the restrictions noted. For rows E and F, 1974 (1978) is dropped All specifications include state and year effects, and the contemporaneous and lagged minimum wage variable, unless otherwise noted. Standard errors treat coefficient estimates, but not means, as random.
[b] Elasticities are evaluated at weighted mean.

existence of either a youth or student subminimum wage, and SMW_{it} is the subminimum wage level (we call the interactive variable the "subminimum wage gap"). Estimates of α_2 greater than zero would support the hypothesis that youth or student subminimums reduce the adverse effects of minimum wages on employment rates for young workers.

We construct the estimate of SMW_{it} in two steps. First, for workers covered by a state minimum wage law, but not the FLSA, a subminimum reduces the wage paid from the level of the state minimum for all workers down to the minimum allowable wage for youths or students. The limited information we have on student and youth subminimum wage provisions suggests that, on average, these provisions permit wage payments equal to about 75% of the minimum wage for other workers. Consequently, for each observation we construct a variable equal to 25% of the state minimum wage level, multiplied by the effective state coverage rate (that is, excluding workers covered by federal law), and divided by the mean wage in the state.[28] Second, in states with a minimum wage level above the federal level, a subminimum wage provision would reduce the wage paid to workers covered by the FLSA from the state minimum wage level to the greater of the federal level or 75% of the state level. For these states we add a second term that is the smaller of 25% of the state level and the difference between the state and federal levels, all multiplied by federal coverage and divided by the mean wage in the state.

Results for teenagers, to whom these subminimum wage provisions are most likely to apply, are reported in Table 7. We report results separately for student/apprentice subminimums, for youth subminimums, and for the existence of any form of subminimum. Consistent with the previous estimates of the minimum wage equation, we report results using contemporaneous and lagged values of the minimum wage and subminimum wage variables. Because this may be asking too much of the data, however, we also report results for more parsimonious specifications that include only lagged values of each variable.[29]

None of the specifications provide statistically significant evidence that student subminimum wage provisions moderate the disemployment effects of minimum wages (columns (1)–(4)). When information on youth subminimums is incorporated into the specifications (columns (5)–(8)), however, there is statistically significant evidence that state youth subminimum wage provisions moderate the disemployment effects for teenagers.[30] In the specifications with the contemporaneous and lagged minimum wage and subminimum wage variables, the positive effects of subminimum wages are significant at or near the 10% level, and in the parsimonious specifications, the effects are significant at the 5% level in three of four cases.[31]

To interpret the coefficients, consider as an example a state with its minimum wage level set above the federal level in 1989 (say, at $4.00 per hour), with full federal coverage, but without a subminimum wage provision. In this case, the full disemployment effect from the state minimum wage is ($\alpha_1 \times 4.00$); using the coefficients in column (8), the employment rate for teenagers is 0.84 percentage point below what it would be in the absence of a minimum wage. If the state enacts a youth subminimum wage provision that lowers the youth minimum to the federal level ($3.35 in 1989), the disemployment effect will be reduced to ($\alpha_1 \times 4.00$) + ($\alpha_2 \times 0.65$), or 0.55 percentage point. Thus, in this example, the introduc-

[28] There were two cases in which states with minimum wage levels below the federal level had a subminimum wage provision on the books, but our estimate of state coverage was zero. In these cases we treated the observation as if there was no subminimum.

[29] For the youth subminimum or "any" subminimum specifications, which reveal some statistical evidence of subminimum wage effects, the restriction dropping the contemporaneous variables is never rejected at the 10% significance level, whereas the restriction dropping the lagged variables (instead) is rejected in two out of four cases

[30] One reason for the absence of significant effects for student subminimums could be that these subminimum wage provisions vary widely across states in terms of the students to whom they apply, with distinctions based on full-time or part-time student status, time of the year (whether school is in session), and so on In contrast, youth subminimum wage provisions are more uniform In addition, student subminimums may apply to some young adults, reducing the ability to substitute teenagers exempted from the minimum wage, or might lead to substitution between students and nonstudents within the teenage group, which would be masked in our employment rates for all teenagers

[31] In contrast to our earlier analyses, in the construction of the subminimum wage gap we utilized information on state minimum wage coverage To ignore state coverage would entail treating states with minimum wage levels at or below the federal minimum wage level identically, whether or not they had subminimum wage provisions That is, the subminimum wage effect would be identified solely from the high minimum wage states This model misspecification seems worth avoiding, despite the measurement error in state coverage For purposes of comparison, we recomputed the specifications in Table 7 using only federal coverage This recomputation resulted in insignificant coefficients for the subminimum wage variables; p-values for the significance of the youth or "any" subminimum wage variables were concentrated in the range from 0 2 to 0 4

Table 7. Within-Group Estimates of Effects of Subminimum Wage Provisions on the Employment-to-Population Ratio of Teenagers.[a]
(Standard Errors in Parentheses)

Independent	Student/Apprentice Subminimums				Youth Subminimums				Any Subminimums			
Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*	*(11)*	*(12)*
Minimum Wage	.09	– 12	—	—	05	– 11	—	—	08	–.12	—	—
	(12)	(08)			(.12)	(.08)			(.12)	(.08)		
Minimum Wage, Lagged One Year	–.13	–.12	– 09	–.17	– 15	–.16	–.14	–.21	–.17	– 15	–.14	– 20
	(.12)	(.08)	(.10)	(.07)	(.12)	(.08)	(.11)	(.07)	(.12)	(08)	(.11)	(07)
Subminimum × Subminimum Wage Gap[b]	04	02	—	—	.39	–.00	—	—	.24	.06	—	—
	(30)	(20)			(.32)	(.21)			(.30)	(.20)		
Subminimum × Subminimum Wage Gap, Lagged One Year	– 15	.06	–.16	12	.27	.43	.48	44	.44	.39	.56	44
	(.37)	(.25)	(.30)	(.20)	(.37)	(.25)	(.32)	(.21)	(.35)	(.23)	(.29)	(20)
Proportion of Population in Age Group	–.11	– 12	–.10	– 14	–.10	–.13	–.09	–.14	– 09	–.11	–.08	–.13
	(24)	(16)	(.23)	(16)	(23)	(.16)	(.23)	(.16)	(.23)	(.16)	(.23)	(16)
Prime-Age Male Unemployment Rate	– 53	–.29	–.53	–.30	–.54	–.31	–.55	–.31	– 54	–.30	–.54	– 31
	(.11)	(08)	(.11)	(.08)	(11)	(.08)	(.11)	(.08)	(11)	(.08)	(.11)	(.08)
Proportion of Age Group in School	—	–.77	—	–.77	—	– 77	—	–.77	—	–.77	—	– 76
		(.03)		(.03)		(.03)		(.03)		(.03)		(03)
Significance of Subminimum Wage Variables[c]	91	.92	59	53	11	.11	.11	.03	07	.07	04	.02
$\bar{R}^2$	.70	86	.70	86	.70	86	70	.86	70	.86	70	86

[a] The sample covers the 50 states and Washington, D C For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974–89, for the remaining states it covers the period 1978–89. The minimum wage variable is the greater of the state or federal minimum wage level, multiplied by federal minimum wage coverage for the state, divided by the average wage in the state. The prime-age male unemployment rate is for men aged 25–64. All specifications include state and year effects

[b] The minimum wage gap is defined as follows: for all observations, it includes 25% of the state minimum wage level, multiplied by state coverage (i.e , the proportion of workers covered by the state minimum wage, but not the FLSA), divided by the mean wage; for observations with minimum wage levels greater than the federal level, this is added to the smaller of either 25% of the state minimum wage level or the difference between the state and federal minimum wage level, multiplied by federal coverage, divided by the mean wage.

[c] p-value for likelihood-ratio test.

tion of the subminimum provision reduces the disemployment effect of the minimum wage by about 35%.

Given the evidence against the use of subminimum wage provisions in one labor market reported by Katz and Krueger (this issue), we considered more carefully the possibility that the results reported in Table 7 represent a spurious correlation. For example, subminimum wage provisions could simply coincide with relatively high employment in particular states, perhaps because states with subminimum wage provisions also have relatively lax enforcement of state minimum wage laws. To examine this question, in Table 8 we report estimates of the same specifications as in Table 7, but only for individuals aged 20–24. Because youth subminimums typically apply to individuals aged 18 or less, if the youth subminimums reduce the adverse effects of the minimum wage on the employment of teenagers, they should have little effect on, and perhaps magnify, the employment losses of those aged 20–24. On the other hand, if the findings reflect a spurious correlation between youth subminimums and employment rates, we might expect to find a similar positive association for these older youths.

In Table 8, the point estimates of the effects of subminimum wages on the employment rates of 20–24-year-olds are generally negative and, at the least, do not replicate the patterns found for teenagers. This result suggests that youth subminimums do moderate the disemployment effects of minimum wages on teenagers. On the other hand, the results for young adults suggest partial substitution away from young adults toward teenagers, which would imply that the moderation of

Table 8. Within-Group Estimates of Effects of Subminimum Wage Provisions on the Employment-to-Population Ratio of 20–24-Year-Olds.[a]
(Standard Errors in Parentheses)

	Youth Subminimums			
Independent Variable	*(1)*	*(2)*	*(3)*	*(4)*
Minimum Wage	−.04 (.10)	−.07 (.09)	—	—
Minimum Wage, Lagged One Year	−.22 (.11)	−.18 (.09)	−.24 (.09)	−.21 (.08)
Youth Subminimum × Subminimum Wage Gap[b]	−.02 (.27)	−.07 (.24)	—	—
Youth Subminimum × Subminimum Wage Gap, Lagged One Year	.08 (.31)	−.20 (.27)	.07 (.27)	−.23 (.24)
Proportion of Population in Age Group	.51 (.14)	.35 (.12)	.50 (.14)	.34 (.12)
Prime-Age Male Unemployment Rate	−.59 (.09)	−.62 (.08)	−.59 (.10)	−.62 (.08)
Proportion of Age Group in School	—	−.77 (.05)	—	−.77 (.05)
Joint Significance of Subminimum Wage Variables[c]	.97	.55	.79	.32
$\bar{R}^2$	.56	.67	.56	.67

[a] The sample covers the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974–89; for the remaining states it covers the period 1978–89. The minimum wage variable is the greater of the state or federal minimum wage level, multiplied by federal minimum wage coverage for the state, divided by the average wage in the state. The prime-age male unemployment rate is for men aged 25–64. All specifications include state and year effects.

[b] The minimum wage gap is defined as follows: for all observations, it includes 25% of the state minimum wage level, multiplied by state coverage (i.e., the proportion of workers covered by the state minimum wage, but not the FLSA), divided by the mean wage; for observations with minimum wage levels greater than the federal level, this is added to the smaller of either 25% of the state minimum wage level or the difference between the state and federal minimum wage level, multiplied by federal coverage, divided by the mean wage.

[c] p-value for likelihood-ratio test

overall disemployment effects is less than that for teenagers alone.

As a second means of studying the validity of the subminimum wage results, we examined wage distributions for teenagers, to see if there are spikes at the subminimum wage, below the minimum wage level. We extracted information on wages from monthly CPS files for 1989, restricting attention to those states with legislated minimum wages above the federal minimum wage; in other states, only workers exempted from federal coverage would be expected to be found at the subminimum.

For the six New England states, there is no evidence of spikes at the subminimum, with the possible exception of Vermont. However, these are high-wage states. In the New England states excluding Vermont and Maine, over 80% of teenagers earn wages higher than four dollars per hour. In Alaska, Hawaii, and California, also high-wage states, there are no spikes at the subminimum.

The lowest-wage states are Pennsylvania, Minnesota, and Washington, in which fewer than 60% of teenagers earn wages exceeding four dollars. As Figure 1 shows, in all three of these states, there are spikes at the subminimum wage. The spike for Minnesota is particularly noteworthy. Minnesota's subminimum ($3.47) exceeds the federal minimum ($3.35), and the spike appears at $3.47, suggesting that the spikes we find at the subminimums for some states do not simply reflect workers covered by federal but not state legislation, or some fraction of employers ignoring state, but not federal, minimum wage laws. Overall, we interpret these data on wage distributions as evidence that the subminimum wage regressions reflect true effects; for some states—in particular, those with relatively low wages—subminimum wage provisions do induce spikes at the subminimum, suggesting that these provisions have a real impact.

Conclusion

Using a specially constructed panel data set on state minimum wage laws and labor market conditions, we have presented new evidence on the effects of minimum wages on the employment of teenagers and young adults, and assessed the extent to which youth or student subminimum wages reduce the adverse disemployment effects of minimum wages. Our reexamination of the existing evidence provides a range of estimated elasticities of employment-to-population ratios with respect to minimum wages. For teenagers (aged 16–19), we find elasticities between -0.1 and -0.2, using correctly specified models (Table 5); that is, a 10% increase in the minimum wage appears to be associated with a 1% to 2% decrease in employment of teenagers. In specifications taking account of school enrollment rates, the results suggest an elasticity closer to -0.2 than -0.1. For young adults (aged 16–24), our best estimate of the range is from -0.15 to -0.2. In general, our results support the consensus of negative effects suggested by the earlier time-series evidence surveyed by Brown et al. (1982), although, as in that survey, it is possible to point to some evidence to the contrary.

In contrast, the results Card reports in his two papers published in this issue appear to conflict directly with our results, since they are based on similar statistical experiments for similar units of observation. In fact, our results do not differ significantly for the one specification that appears in both Card's papers and this paper: teenage employment specifications that do not control for school enrollment rates. We argue, however, that this model is misspecified by the exclusion of the school enrollment rate. Moreover, we show that the failure to consider lagged effects of minimum wages, especially in the short first-difference estimators used by Card, results in substantial upward bias in the estimated effects of minimum wages on employment, leading to elasticities that are too close to zero, and frequently positive. For the other specifications we

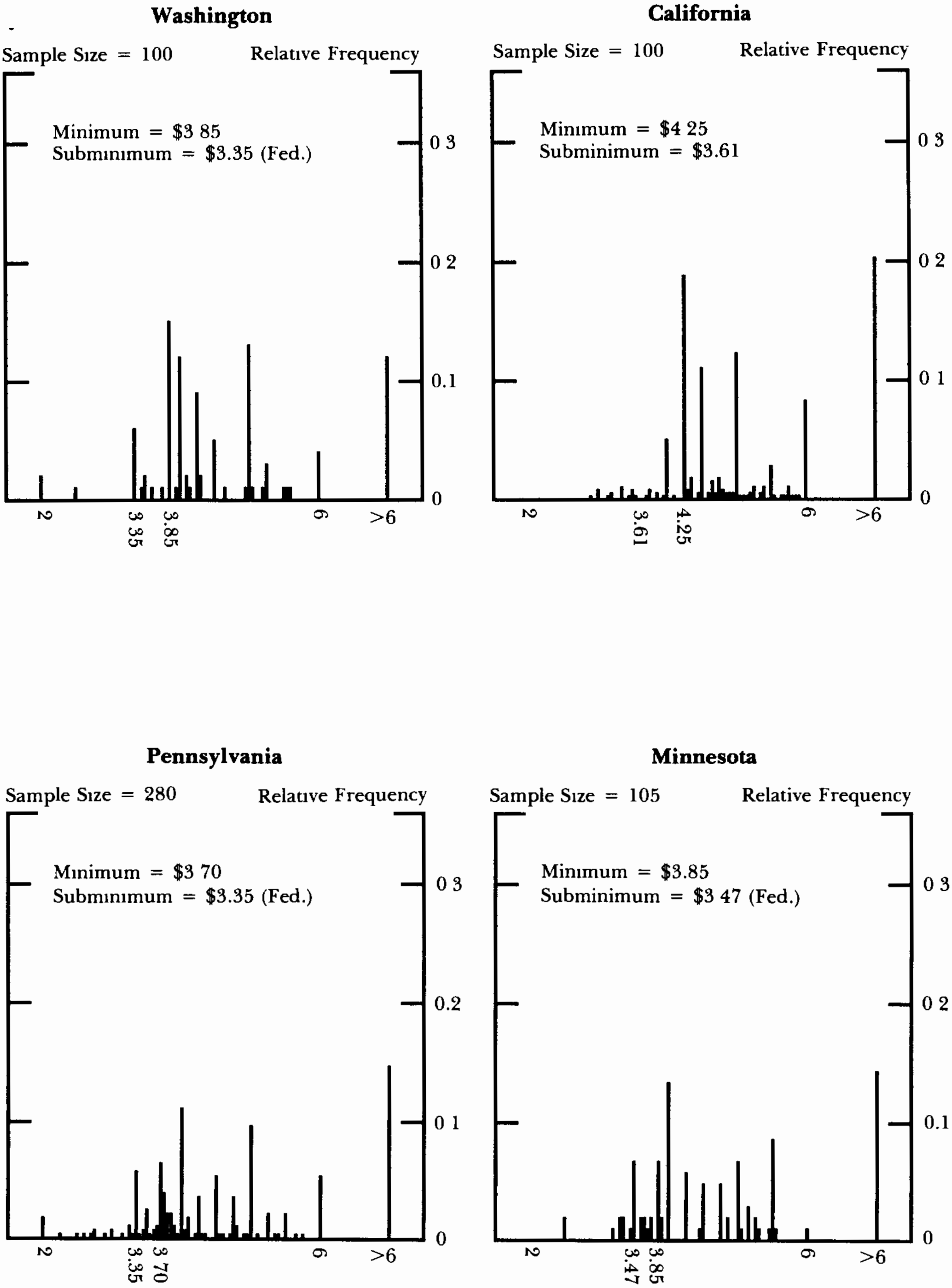

Figure 1 Wage Distributions for Teenagers (Ages 16–19) in High Minimum Wage States, 1989.

estimate (a specification that includes school enrollment rates in the equation for teenagers, and both specifications for young adults), the evidence from correctly specified models points to consistently negative effects of minimum wages on employment, with negative long-run elasticities in the ranges reported above.

The apparently anomalous result for teenagers in specifications that do not control for school enrollment rates suggests that further research that controls for school enrollment rates, while attempting to correct for their potential endogeneity, will be fruitful. In addition, it would be informative to explore the reasons for the lag between the enactment of minimum wage hikes and the employment changes that result; an interesting project for such research might exploit cross-state variation in the length of periods between enactment of minimum wage legislation and the actual increases, paralleling time-series experiments in Brown et al. (1983).

Based on our preferred specifications, we also provide estimates of the role of subminimum wages in reducing the adverse effects of the minimum wage. Our results indicate that youth subminimums, but not student subminimums, moderate the disemployment effects of minimum wages on teenagers. We present additional evidence that, in our data, the effects of subminimum wage provisions are not spurious. This result stands in contrast to the puzzling finding, reported in Katz and Krueger (in this issue), that fast-food restaurants in Texas did not utilize the federal subminimum wage introduced in 1990. But research on subminimum wages is in its infancy, and a more definitive answer awaits further research.

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